Personal Memo from
JOSHUA LEDERBERG

Prof. Harland Wood

SEP - 6 1985

[redacted] I find fascinating to read of your own C^{13} fractionation and building a mass-spec.

As you mention Ed Tatum: do you have any notes or reminiscences to add to the enclosure?

Did he ever mention Carrol for you at Wisconsin? Did anyone to your recollection prior to 1941?

Yours, with best wishes

Joshua Lederberg

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THE ROCKEFELLER UNIVERSITY NEW YORK 1002

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the supplement; our second daughter was born in Madison. There Ed Tatum was biding time waiting for a job or a postdoctoral position. Later, he received a fellowship with Kögl in the Netherlands; following that, he joined George Beadle in studying the genetics of *Neurospora,* which led to the Nobel Prize. Wisconsin then was a center for nutritional studies, and Tatum and I investigated the growth factor requirements of propionibacteria. We (7) showed for the first time that vitamin B_1 is required for the growth of a microorganism, the propionic acid bacteria. My experience at the University of Wisconsin broadened my outlook on science. I had many stimulating discussions with, among others, Marvin Johnson, Perry Wilson, and Wayne Wooley; I also met Esmond Snell, who was then a graduate student.

Professor Werkman offered me a position as an Assistant Research Professor. I had written to Otto Meyerhof hoping to spend the second year of the fellowship in his laboratory. Jobs were few and far between so I accepted Werkman's offer. I have often wondered how a year with Meyerhof would have influenced my career. Almost certainly I would have received useful training in enzymology there. I remained at Iowa State University for seven years.

LIFE AT IOWA STATE UNIVERSITY

An Introduction to the Stable Isotope of Carbon, ^{13}C

There was an International Congress of Microbiology in New York City in 1939. There I first learned about ^{11}C and of its availability in Berkeley. At that time, we had indirect evidence that the CO_2 is fixed in succinate. It was clear with $^{11}CO_2$ that this could be proved conclusively. I was told if I could obtain the succinate in about 5 hr, it would be possible to do the experiment. The timing was necessary because ^{11}C has a half-life of 20.5 min. I started work immediately and found if I used a thick suspension of washed cells I could ferment glycerol rapidly. Then, I acidified the mixture and mixed it with plaster of paris, thus obtaining a dry powder. The succinic acid was extracted by pouring ether through the powder, and the succinate was isolated as the calcium salt in good yield in an overall time of less than 4 hr. I told Professor Werkman of my success and that I planned to drive to Berkeley that summer at my own expense. I was amazed when he said, "No, you can't go." He never offered an explanation, but my guess is that he thought we would lose control of the problem since we didn't have access to ^{11}C.

I doubt if Professor Werkman deserves any credit, but as events turned out, it was a fortunate decision. That summer, at Lake Washington, I told my brother Earl about this incident. He was studying for his PhD and M.D. degrees at the University of Minnesota, and he knew about studies that were being initiated with ^{13}C, the stable isotope of carbon. He told me ^{13}C was being